The question for us is the effectiveness of the material as an aid to learning.

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Ultrasound of the gallbladder

SIR,—We would like to express our agreement with the article by Dr T M Walker (2 May, p 1452) on the value of ultrasound in biliary disease. Since 1978, when this hospital obtained an ultrasound machine, our policy in managing acute cholecystitis changed from conservative treatment followed by cholecystography and elective operation (group 1) to urgent ultrasound followed by urgent surgery (group 2). We have since reviewed the results of 39 cases. There were 15 patients in group 1, eight of whom showed a non-functioning gallbladder. In the 24 patients of group 2 there were three false-negatives and one false-positive in the diagnosis of gall stones. Complications after surgery for the two groups were similar. The numbers are small but do show that ultrasound is a good method of diagnosing acute cholecystitis, thus enabling the surgeon to perform urgent surgery and reduce the patient's suffering and hospital stay.

Previously one of the disadvantages of urgent surgery for cholecystitis was that a cholecystogram could not be used to confirm the diagnosis until six weeks after the acute attack. The introduction of ultrasound has removed this problem. If used with an adequate history and examination, ultrasound is a useful investigation in acute biliary disease.

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SIR,—We would like to support Dr T M Walker (2 May, p 1452) in his contention that a satisfactory ultrasound examination of the gall bladder does not necessarily require a "strongly motivated and highly experienced ultrasonographer," as you suggested in a leading article (3 January, p 3).

After two months' experience using an Aloka real-time ultrasound machine, one of us (JMD, a radiographer) provided biliary tract ultrasound pictures, which were then read by a radiologist. We have compared this examination in 159 patients with an oral cholecystogram carried out at the same time. If on a plain radiograph of the gall bladder area calcified stones were seen, then these patients were excluded. Positive identification of gall stones was obtained in 48 cases using ultrasound. With the oral cholecystogram, in 29 cases gall stones were seen and 15 non-opacified gall bladders were also found. Of the non-opacified gall bladders, ultrasound showed stones in 12. Furthermore, ultrasound showed in two patients hepatic metastases, and in two others renal cysts.

We would like to stress the value of a modern, high-resolution real-time ultrasound machine in gall bladder examinations. We have found the examination of the gall bladder with this machine quicker and, we believe, more accurate than with a B-scan machine. Compared with oral cholecystography, real-time ultrasound demonstrated stones more frequently, it costs less, and there is no hazard from *x*-rays or from oral contrast medium. The patient spends a shorter time in the department.

For 50 years the oral cholecystogram has been the primary means of imaging the gall bladder. We have now abandoned the oral cholecystogram as our primary examination and use real-time ultrasound.

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SIR,—The doubts that Dr T M Walker describes in his article on ultrasound of the gall bladder (2 May, p 1452) are shared by all of us trying to adapt to a simpler, cheaper method to confirm a very common clinical diagnosis. Most surgeons, with prompting from their radiologists, are after a time happy to accept the echoes described as stones. However, with a cholecystogram we know the size of the stone that we are dealing with and we are now often surprised and unprepared for the very small stones that give good echoes on the ultrasound examination. Previously, many patients obviously had small particles in their gall bladder which caused their initial symptoms. We treated these patients on a low-fat diet and frequently they became free of symptoms. In the future, if surgeons are to use this as a routine method of investigation we will need further assistance from the radiologists to give us a measurement of the diameter of the stones seen. If they can do that most of us will ask for an ultrasound rather than a cholecystogram and be happy and confident with the result. If not, ultrasound will become just another extra test.

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"General Practice Revisited"

SIR,—I should like to make the following comments on the points raised by Dr Ann Cartwright and Mr Robert Anderson (16 May, p 1625) about my review of their book *General Practice Revisited* (18 April, p 1291).

(1) As I said, *in the main text* they do not relate the rather oddly chosen terms middle class and working class to the Registrar General's classification. What is revealed in appendix V is a very arbitrary, or at any rate unexplained, division into social classes I, II, and III non-manual on the one hand and III manual, IV, and V on the other, when most health services research uses the convention I and II, III non-manual and manual, IV and V; and there is considerable evidence that the important cut-off is between I, II, and III on the one hand and IV and V—groups that are in many ways disadvantaged—on the other.

(2) I accept the author's assurance that the question asked did make sure the respondent understood that the question referred only to surgery appointments.

(3) On page 5 in a footnote the authors say that "attention is not drawn to differences that might have occurred by chance five or more times in 100." From this the reader could infer that such differences would not be discussed in the text. In appendix IV, however, they say, "There are a number of factors, particularly the nature of the data and the stage at which precise hypotheses were often formulated, that violate some of the conditions in which statistical tests of significance apply, and make interpretation difficult. . . . Chi square, *t*, chi square trend tests, and tests for differences between proportions have been applied constantly when looking at the data from this survey and have influenced decisions about what differences to present and what verbal 'weight' to attach to them." Since the tables do not show statistical significance, is one to assume that unless it is specifically stated none of the differences is statistically significant? If so, how can one evaluate the importance attached to them by the authors, or guess at the "verbal weight"? For example, in the section (in the chapter "The Setting") on partnerships the authors make more than 20 statements on differences or comparisons before saying, "The only statistically significant differences were that singlehanded doctors were more likely to say that there was nothing about their work which they found frustrating, but fewer of the singlehanded doctors mentioned the variety of unpredictability of the work as something they enjoyed, and fewer of them commented on their freedom or independence." What weight should the reader attach to the previous statements?

(4) An example of a table on which the reader cannot do tests of significance is table 8, which looks like a simple contingency table until one reads in the text below, ". . . fewer people felt it did not matter." Surely these should have been included in the table?

(5) and (7) The authors say, "Further evidence that patients' attitudes to drugs had changed comes from the nature of the criticism of *their* doctors' prescribing habits" (my italics), and show that those who thought that their doctor was too inclined to give a prescription had increased from 2% to 7%. When 89% of the population think that their doctors are reasonable about this it is difficult to argue that the change is important. When asked the entirely hypothetical question, unrelated to personal experience, about doctors in general, 46% thought that they were too inclined to prescribe. We are not told how this compares with 1964 so that we are unable to decide whether this much "softer" datum supports the authors' contention that "attitudes to drugs had changed." The tenor of *their* commentary suggests, however, that in discussing the public's attitudes to doctors' prescribing they prefer to use those which are not based on personal experience rather than those that are. The *statistical* significance of the change from 2% to 7% for those who thought their doctor too ready to prescribe is, of course, high (although slightly weakened if the 127 non-responders in 1977, not shown in the table, are included). It is doubtful, however, whether this finding, thus shown to be very unlikely to be by chance, has any *practical* significance: doctors are not faced with a tidal wave of scepticism about their prescribing.

(6) I erred in saying that the availability of a social worker was related to the proportion who thought it inappropriate to bring family problems to the doctor: what it *did* correlate with was the feeling that a high proportion of the work was trivial.

(8) I said that the doctors' estimate of the proportion of their consultations that are trivial has decreased, whereas the authors say that there was no difference in the proportion of consultations that were trivial, and in both studies the average was a third. From the table, however, it can be seen that, whereas in 1964 26% of the doctors interviewed thought more than half their work was trivial, in 1977 only 24% of doctors did: and in 1964 44% of the doctors thought that less than a quarter of their work was trivial, but in 1977 this had risen to 50%. My guess is that the median may have fallen while the mean has not.

I am, of course, sorry if my comments have upset the authors, whose work I respect and whose data, as I said, will be very useful to those engaged in health services research. They challenge eight statements in my review, and in two cases I admit error and apologise; in the other six I think my comments were appropriate. What is surprising is the criticism that they did not challenge.

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